APPENDICES to "*Biotic SOC Stock: What We Had & What We Lost*"

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OVERTURE

https://www.youtube.com/watch?v=L_Dv2LBPw9E&ab_channel=ITServicesCEO.

APPENDIX A

Prehistoric and Historic Rates of SOC Losses

On 11th November, 2022, following latest ESSD (2022) Global Carbon Budget Report (Friedlingstein et al. 2022 - https://doi.org/10.5194/essd-14-4811-2022), new compilations by https://ourworldindata.org/co2-data-update-2022 used data sourced from the Global Carbon Project (www.globalcarbonproject.org/carbonbudget/22/data.htm). Their new total cumulative emissions values for the World in 2022 (https://ourworldindata.org/explorers/co2) are: From Fossil Fuels & Industry since 1750 at 1,700 Gt CO_2 (x 0.27 = 459 Gt C); due to Land Use Change from 1850 to 2022 at 742.5 Gt CO_2 (x 0.27 = 200.5 Gt C for combined total of 659.5 Gt C); and their combined LUC & FF from 1850 to 2021 at 2,470 Gt CO_2 (x 0.27 = 667 Gt C). A slight totals discrepancy of 667 - 659.5 = 7.5 Gt C is only partly accounted for by an extra 4.76

Gt CO_2 (= 1.3 Gt C) in the FF value from 1750-1850 (cf. about an extra 29 Gt from 1750-1850 from other sources e.g. Gasser et al. (2020 https://bg.copernicus.org/articles/17/4075/2020/) found cumulative total of 206±57 Gt C ver the 1750–2018 period while also estimating that land cover change induced a global loss of additional sink capacity – that is, a foregone carbon removal, not part of the emissions – of 0.68±0.57 Cg C/ y and 32±23 Gt C over the same period.

Increase of atmospheric CO_2 from 285 ppm in 1850 to 415 ppm today is about +276 Gt C.

ESSD's latest Global Carbon Budget (Fredlinstein et al. 2022 - https://essd.copernicus.org/articles/14/4811/2022/) provides similar "official" FF emissions from 1850-2021 of 465 ± 25 Gt C; LUC emissions from 1850-2021 of 205 ± 60 Gt C (total cumulative anthropogenic total of 670 ± 65 Gt C) and atmospheric CO_2 accumulation increase of 48 % from (590.5 Gt C in 1750) or from 605 Gt C in 1850 to 879 Gt C in 2021 is by ~274 Gt C. Leading to assumption 670 Gt C emission total comprises ~205 (30%) from LUC, ~465 (70%) FF.

From 1750 or 1850 ESSD (2022) provide an atmospheric CO_2 of 278.3 ± 3 ppm or 285.1 ± 3 ppm, respectively (a difference of about 6.8 ppm or x 2.124 = 14.4 Gt C), since atmospheric CO_2 in 2022 is about 415 ppm, the difference from 1850 is about +130 ppm or (x 2.124 =) +276 Gt C.

Thus conclusion on human CO_2 emissions from 1850 in Gt C are a third (670/205 = 31.6%) from LUC, two thirds (670/465 = 69.4%) from FF, with almost half (670/276 = 41%) CO_2 accumulated.

However, while total fossil fuel values may easily be accounted for, LUC is more complex.

Following Blakemore (2018b), LUC calculations based upon planimetrically flat biomes are at least doubled for terrain. A new LUC value since 1850 is around 410 Gt C that, when added to a corresponding FF estimate of 465 Gt C, is 875 Gt C total emissions (+5Gt C/yr). This value is 31% higher than previous 670 Gt C total emissions (+4 Gt C/yr), yet unchanged atmospheric CO_2 increase of +276 Gt C may be accounted for by an accompanying 31% increase in terrestrial NPP due to a CO_2 fertilizing effect and warmer land (Haverd et al. 2020).

These values are reassessed and compared to CarbonBrief (www.carbonbrief.org/analysis-which-countries-are-historically-responsible-for-climate-change/) summary that: "*Land-use change and forestry added some 786GtCO2 during 1850-2021, amounting to nearly a third of cumulative total, with the remaining two-thirds (1,718GtCO2) from fossil fuels and cement*." This is 212 LUC + 464 FFs to total 676 Gt C. Statista

(https://www.statista.com/statistics/1267683) similarly have: "*Humans have released approximately 2,500 billion metric tons of carbon dioxide (GtCO2) emissions into the atmosphere since 1850 from fossil fuel combustion and land-use*"; total 2,500 x 0.27 = 675 Gt C. Similar sums were provided by IPCC latest report (IPCC, 2022 - www.ipcc.ch/report/ar6/wg3/downloads/report/IPCC_AR6_WGIII_SPM.pdf): "*cumulative net CO_2 emissions from 1850 to 2019 were 2400 ± 240 $GtCO_2$*"; (x 0.27 =) 648 ± 65 Gt C.

An extra ~29 Gt C in LUC value from 1750-1850 is derived from Glasser et al. (2020 - https://bg.copernicus.org/articles/17/4075/2020/bg-17-4075-2020.pdf) LUC totals of 206 ± 57 Gt C from 1750 to 2018 and 178 ± 50 Gt C from 1850 to 2018 that they say is consistent with the GCB2019 estimates of 235 ± 75 and 205 ± 60 Gt C over the same periods, or +28 and +30, respectively (ESSD 2019: tab. 8 https://essd.copernicus.org/articles/11/1783/2019/).

Prior to 2022, OurWorldinData (https://ourworldindata.org/contributed-most-global-co2) found "*Since 1751 the world has emitted over 1.5 trillion tonnes of CO_2*"; 1,500 Gt CO_2 (x 0.27 =) 405 Gt C which they later state is only fossil fuels and industry, not land use change. Their cumulative World total in 2021 was somewhat higher at 1,736,930,100,000.00t CO_2 (https://ourworldindata.org/grapher/cumulative-co2-emissions-region?tab=table) or 1,737 (x 0.27 =) 469 Gt C FF total. Importantly, for their sources (https://ourworldindata.org/co2-dataset-sources) they state: "*Until the 2022 update of the Global Carbon Budget, national emissions data was only available for CO_2 emissions from fossil fuels and industrial processes (such as cement production). It did not include emissions from land use change. The 2022 update – for the first time – includes land use estimates for countries, extending back to 1750.*"

Nevertheless, ~660 Gt C total emissions comprise ~200 (30%) due to LUC and ~460 (70%) FF.

Buringh (1984) estimated a total 537 Gt SOC loss in the last two millennia. Comparatively, Lal (2004: tab. 1) had SOC lost due to carbon emissions from Land-use conversion (LUC) in a Preindustrial era at about 320 Gt C, and LUC in a Postindustrial era (since 1850) at 136 Gt C. Lal (2004: tab. 1) footnoted that Ruddiman's (2003) estimation of emission from Postindustrial land-use conversion was higher at 0.8 Gt C/year for 200 years at 160 Gt SOC loss. Both values are less than the ~200 Gt SOC from later LUC studies. Total LUC of 456-520 is a bit less than Buringh's value of 537 Gt C that is itself about double Lal's estimate of Fossil-fuel combustion since Industrial era of 270 Gt C (now about +280 Gt C). From this data alone of 456-537 Gt C

loss it is surmised that, rather than a lesser amount, total historical SOC loss emitted up to twice as much CO_2 carbon as post-industrial fossil fuels. The current study finds higher totals.

Since publications, rates of ~0.1 Gt C/yr add 42 Gt C to Buringh's and 22 Gt C to Lal's values.

Preindustrial or Holocene C emissions from 8,000 years ago to the beginning of large-scale industrialization (~1850) were modelled to range just 326-357 Gt C (median ~340 Gt C) by Kaplan et al. (2011). Houghton (2003) estimated LUC released 156 Gt C to the atmosphere from 1850–2000, equivalent to about 57% of fossil fuel emissions in the same time period. Combined values are 340 pre- + 156 post-industrial = 496 Gt C in total lost from soil from LUC.

Conversely, a much lower claimed "*soil carbon debt*" since the beginning of agriculture 12,000 years ago yet accelerating in more recent history with industrialization and intensification was just 133 Gt C (later "corrected" further down to 116 Gt C), about two thirds, or ~88 Gt C, to the atmosphere that may theoretically be recoverable according to Sanderman et al. (2017; 2018). Giving previous cumulative SOC loss for the world's agricultural land estimates ranging from 40 to over 500 Gt C, these authors quote between just 30-62 Gt SOC loss for post-1850 industrial period. This is a wide disparity compared to Buringh, Lal or Ruddiman's 136-250 Gt SOC values.

Similarly, Wang & Van Oost (2019) had also suggested a much lower present-day sedimentation accelerated cropland soil erosion at ~11.7 Gt/yr. This compares to an earlier estimate of agricultural sediment flux on cropland at 22 Gt/yr plus an additional 11 Gt/yr mobilized on pasture or rangelands (total soil 33 Gt/yr) with carbon erosion flux from cropland SOC of 0.32 Gt C/yr and total agricultural SOC erosion just 0.47-0.61 Gt C/yr (van Oost et al. 2007). These are one tenth of Lal's (2006: fig. 3.2) values figured above of 4-6.1 Gt C/yr.

Replying to the latter paper, Lal & Pimentel (2008) argue for higher values saying: "*soil erosion is a strong source rather than sink of atmospheric CO_2 , and it also exacerbates the problem of nonpoint source pollution and hypoxia in coastal zones*". Data presented herein support this.

FAO (**2015**a: 101; **2015**b) soil erosion estimate of ~33 Gt/yr corresponds to earlier erosion figures by Van Oost et al. (**2007**) of about -22 Gt/yr from cropland and another -11 Gt/yr from pasture and rangelands (total -33 Gt/yr that other authors say are equivalent to -1.0 and -0.3 or total -1.3 mm/yr). Van Oost et al. (**2007**) say these soil losses correspond to cropland SOC erosion of just -0.32 Gt C/yr and a total agricultural SOC erosion rate of only about 0.5 Gt C/yr that, doubled for terrain, would be about 1 Gt C/yr.

Lal (**1995**: fig. 2, pg. 141) has much higher rates of soil erosion, but with the caveat that much is just redistributed on land. His total erosion is about 190 Gt dry soil/yr with 19 Gt transported to the oceans. He has 5.7 Gt C/yr displaced from soils (about 3% of the topsoil displaced) but only 0.57 Gt C/yr to the oceans (this is about 10% of the carbon displaced). About 4 Gt C/yr of SOC soil organic carbon is redeposited on land (and to rivers/lakes?). He tallies an unrealistic 1,500 Gt SOC stock to 1 m and values may reasonably be doubled for terrain (i.e. 11.4 Gt C/yr).

Lal (2006: fig. 3.2) had 4-6.1 Gt C/yr transported by erosion (about 3-4.3 redistributed, the rest lost). These values are close to those of Buringh (1984: tab. 3.8) who had a range of annual loss of SOC between 2.5-7.4 Gt C/yr with 4.6 Gt C/yr as a realistic mean estimate but based on an unrealistic stock of just 1,477 Gt SOC. Doubled for terrain, range is 5-14.8, median 9.2 Gt C/yr. Some of Professor Lal's calculations are difficult to reconcile, moreover adoption of a mean 2% SOC value implies some underestimation of his totals (cf. 2.5% SOC value as selected herein).

Acid rain plus temperature rises on land affect all soils. A 2015 FAO summary report (https://www.fao.org/3/i5199e/i5199e.pdf) claims in the last 40 years nearly a third of the world's arable land has been lost by erosion and continues to be lost at a rate of more than 10 million hectares per year. Myers (1993), Pimentel et al. (**1995**), Pimentel & Burgess (**2013**: 447) and Pimental & Kounang (1998) conservatively estimate 75 Gt/yr (or ~2,000 tonnes per second!) of fertile arable soil alone is eroded from agricultural lands globally 80% or which suffer moderate to severe erosion (see Blakemore **2017**). This value is supported by FAO GSP (2017 - http://www.fao.org/global-soil-partnership/resources/highlights/detail/en/c/416516/) whereas Borrelli et al. (2017 - https://www.nature.com/articles/s41467-017-02142-7#Bib1) claim a rate half this of just 35.9 Gt/yr.

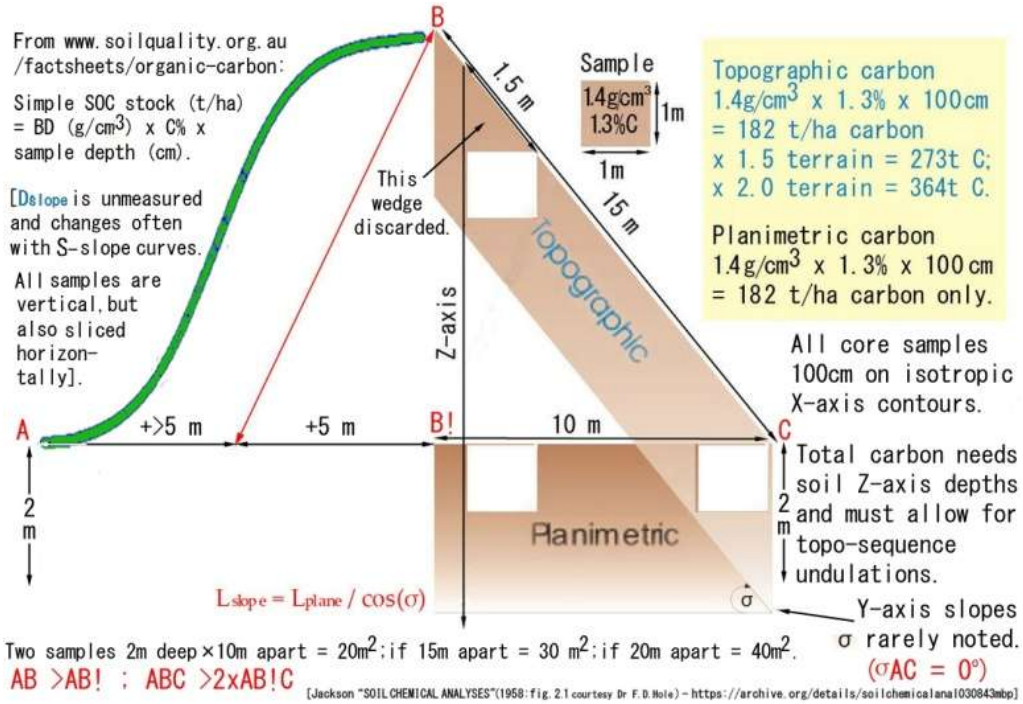
FAO (2015a: 1-1 http://www.fao.org/3/a-i5199e.pdf: 2015b: http://www.fao.org/news/story/en/item/357059/icode/) had global topsoil erosion ranges from 20 to 200 Gt/yr but argue that reasonable figures are ~25 Gt/yr by water, another 5 Gt/yr for tillage and 2–3 Gt for aeolian dust to total ~33 Gt/yr. This is similar to Lal (2020, 2022) erosion of 36.6 Gt/yr at present in the absence of reservoir trapping by water reportedly emits ~1.1 Gt C/yr. He estimated total amount of SOC displaced by sheet erosion from its source was 1.32 ± 0.20 Gt C/yr. Earlier, Lal (2020) had: "*The global magnitude of SOC erosion may be 1.3 Pg C/yr. by water and 1.0 Pg C/yr. by wind erosion*.." (total 2.3 Gt SOC/yr) and that ESSD Global Carbon Budget "*is incomplete and uncertain because SOC erosion is not accounted for*." The

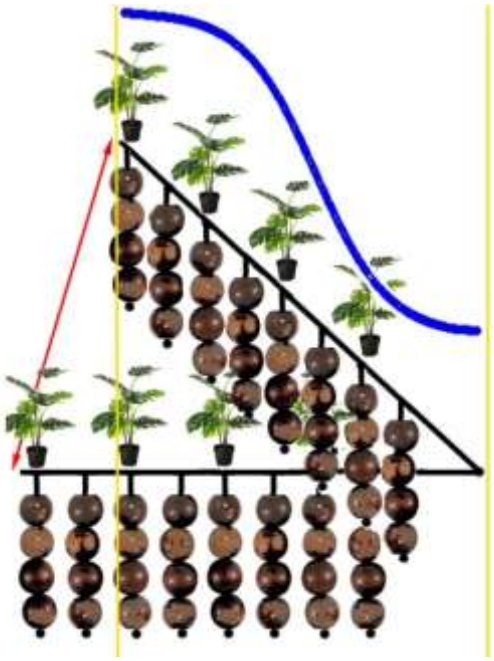
total is 1–2.3 Gt/yr SOC loss that may be doubled for neglected terrain to 2–4.6 Gt/yr SOC erosion. Yet higher values are likely but are not particularly well studied due to lack of general soil research.

Lady Eve Balfour's excellent introduction to the now not-so-excellent corporate IFOAM (Ref.): "*The criteria for a sustainable agriculture can be summed up in one word -- permanence, which means adopting techniques that maintain soil fertility indefinitely; that utilise, as far as possible, only renewable resources; that do not grossly pollute the environment; and that foster life energy (or if preferred biological activity) within the soil and throughout the cycles of all the involved food-chains.*"

Regarding global total SOC values, the estimates in Blakemore (2018b - https://www.mdpi.com/2571-8789/2/4/64) of >8,580 Gt C have yet to be falsified thus may be accepted as reasonable as fully justified (and not just terrain as some claim!). These values were restated and explained in more detail in Blakemore (2019 - https://vermecology.wordpress.com/2019/09/20/soc-upped-after-uninvited-comment-on-soil-syst-2018-2-64/) that includes the four images below of main arrougant French antagonist deniers, Dr Dominique Arrouays and his boss Mr Stephan LeFol, here contradicting each other for some reason (or none..) with their best (false) SOC estimates at 1,417 or 2,500 Gt C!

Comment following my 2018 paper was uninvited (allowable) but misrepresented my work (not acceptable) as they claim samples were collected parallel to undulating soil surfaces whereas it was actually stated more than once that they are taken "*perpendicular to the Centre of the Earth*" and soil samples are actually like the beads on a string-bead curtain being lifted or lowered:-





Please note too how plants, here symbolized by potted ferns, are closer together on slopes. This is what appears true to Nature with forests on slopes more dense than forests on plains.

APPENDIX B extracted from https://www.parliament.vic.gov.au/images/stories/committees/enrc/soil_carbon_sequestration/submission/Australian_Soil_Carbon_Accreditation_Scheme_ASCAS.pdf :

"**APPENDIX B**: Soil carbon levels in the early settlement period Noted Polish explorer and geologist, Sir Paul Edmund [Count] Strzelecki, travelled widely through the colonies of south-eastern Australia during the period 1839 to 1843, collecting minerals, visiting farms and analysing soils. One of the questions Strzelecki posed was, what factors determine soil productivity? He collected 41 soil samples from farmed paddocks of either high or low productivity. The analyses revealed that the most important determinant of soil productivity was the level of soil carbon (measured as organic matter in Strzelecki's day). Of the 41 samples analysed, Strzelecki (1845) found … The top 10 soils in the high productivity group had organic matter levels ranging from 11% to 37.75% (average 20%) The lowest ranking 10 soils in the low productivity group had organic matter levels ranging from 2.2% to 5.0% (average 3.72%) The soils with the highest organic matter levels also had the highest moisture holding capacity, with an 18-fold difference in capacity to hold moisture between the lowest and the highest (Strzelecki 1845). **Strzelecki's data indicate that organic matter levels in the early settlement period were around five times higher than in Victorian soils today (Appendix C). It is also worth noting that Strzelecki's samples were not from virgin soils. They were collected from paddocks that were already being farmed. It is therefore likely that the original organic matter contents of these soils were even higher than recorded.** The soil test data from Strzelecki is consistent with the writings of early settlers. For example, the 1840s journal of George Augustus Robinson (Appendix A) described Victorian soils in the regions to the north and north-west of the Port Phillip settlement as being extremely fertile and productive in the mid-1800s. A Faculty of Agriculture was established at the University of Melbourne in 1905, more than 100 years from the time of the first European settlement at Port Phillip Bay in 1803. Much had changed in the intervening period, not the least of which was the status of Victorian soils. Indeed, the abundant summer-green perennial grasslands and deep, carbon-rich, friable soils described by early pastoralists had long since disappeared. Even under most current 'best management' practices, soils continue to lose their organic carbon. The legacy for current landholders is that input costs for the agricultural sector are rising, with no commensurate increase in productivity. This situation could be reversed by the restoration of landscape function through a return to appropriately managed biodiverse perennial groundcover. Literature cited Strzelecki, Paul Edmund de, 1845, Physical description of New South Wales and Van Diemen's Land : accompanied by a geological map, sections and diagrams, and figures of the organic remains / by P.E. de Strzelecki Printed for Longman, Brown, Green, and Longmans, London. (Note: prior to 1851 the state of Victoria was part of the colony of New South Wales)."

Low, normal and high ranges for average soil organic carbon levels (% by weight) in crop and pasture soils in low rainfall (< 500mm) and high rainfall (>500mm) regions, Victoria

_____ Low rainfall (< 500 mm) High rainfall (> 500 mm) Crop Pasture Crop Pasture Low 0.9 1.7 1.45 1.45

>2.6 >2.9 >5.8

Source: Brown A.J., Fung K.K.H. and Peverill K.K.I. (1980). A manual on the soil testing service provided by the Division of Agricultural Chemistry, Department of Agriculture, Victoria, Technical Report Series No 34.

MUCH OF ABOVE FROM C> JONES ALSO ONLINE HERE - http://www.amazingcarbon.com/PDF/JONES-SoilCarbon&Agriculture%2818May10%29.pdf .

https://www.amazingcarbon.com/PDF/JONES-SoilSequestrationInquiry(17Dec09).pdf : "*Gale and Haworth (2005) calculated that 85% of post-contact erosion occurred in the first 25 years of European settlement. Significantly, a majority of the displaced soils in the early settlement period consisted of low bulk density humus-like material with an organic matter content of around 7%. From 1861, there was a sudden transition to a lower, constant rate of erosion of 52 tonnes per square kilometre per year* [5.2t/ha/yr]. *The authors concluded that the high rates of early colonial soil loss had almost entirely depleted the catchment of erodible material, with the result that erosion moved from a transport-controlled regime to one that was limited by the rate at which catchment material was made available for transport. The situation described by Gale and Haworth (2005) for the Northern Tablelands of NSW was mirrored in many other parts of Australia, including Victoria*."

See also Dr Christine Jones' article here - https://web.archive.org/web/20200315053953/http://www.farminstitute.org.au/_literature_68254/AGE2010_Paper_Dr_Christine_Jones

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INTERLUDE or INTERMISSION

Choices: www.youtube.com/watch?v=YoEqAGwWuaA&ab ; https://youtu.be/wHsfIwNNgLo ; www.youtube.com/watch?v=LAz0uNKw8QI&ab. An Editor and Referee thought the draft paper was too long, thus gentle Readers are welcome hereabouts to freely take a short break.

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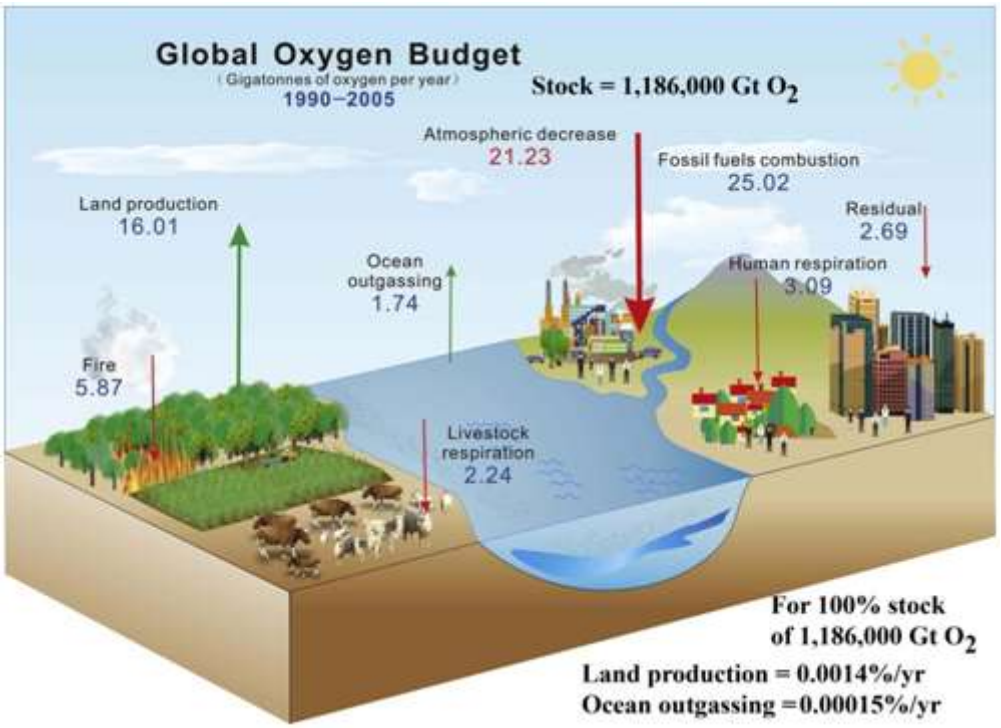
APPENDIX C

Comparative Ocean Oxygen and Carbon Budgets and Cycles

Whereas soils are the major part of an active and rapid global carbon cycle, Oceans get much of their organic and inorganic carbon via coastal erosion, river run-off and aeolian drift from land. Gaseous exchange at water surface is via a passive and instantaneous Henry's Law mechanism rather than the active photosynthesis and respiration events that occur on land. Thus oceanic outgassing of oxygen and carbon dioxide are relatively miniscule, despite many unsubstantiated counter claims (see - https://vermecology.wordpress.com/2021/05/27/h2o/).

For O_2 recycling (Huang et al. (**2018**: fig. 4) about 16 Gt Gt O_2/yr from Land production and just 1.74 Gt O_2/yr from Ocean outgassing, or about 0.0014% from soil and just 0.00015% from sea.



Overall an annual global oxygen deficit (-21.23 Gt/yr) is caused by burning and soil respiration, albeit this is miniscule compared to the atmospheric O_2 stock that comprises 21% of air. E.g. Keeling et al. 2021 say "*The O_2 loss is the flip side of the CO_2 buildup*". Thus false claims by Marine Biologists that Oceans supply 50-90% of the oxygen we breath each year are dismissed.

Looking as Earth's oxygen on a broader canvas, Holland (**2006**) explained: "*The principal cause of the proposed high O_2 levels was probably the evolution of the large vascular **land plants** that brought about increased O_2 production and increased O_2 levels due to the enhanced global burial of microbially resistant, lignin-rich organic matter* [i.e. soil organic matter or humus] *during the Permo–Carboniferous (**Berner 2004**)*." This great glaciations era occurred about 250 million years ago with carbon stored in soil humus, peat, permafrost and various fossil forms.

Since photosynthesis is $6CO_2$ (from air) + $12H_2O$ (from soil) (in sunlight & chlorophyll) = $C_6H_{12}O_6$ + $6O_2$ (to air) + $6H_2O$ giving an effective ratio of CO_2 carbon to atmospheric oxygen (O_2) of 1 : 1. Thus a rough, back-of-the-envelope calculation of stochiometric C:O_2 for current ~1,200,000 Gt O_2 (from **Duursma & Boisson, 1994: tab. 1**) means that if ~80% was due to terrestrial photosynthesis then, from C/O_2 molecular weights of 12/32, about 360,000 Gt C was fixed in humic soils with much since fossilized or eroded to seas! This is a massive soil carbon accumulation (cf. 65,000,000 Gt C stored in rock, limestone, etc. and 5-10,000 in FFs in Fig. 2).

Possibly support for this argument is Sun et al. (2020; fig. 1) showing carbon accumulation accelerating 2 billion years ago from 1 to 2 x 10^{21} moles of organic carbon (= ~12 x 10^6 Gt C).

Incidentally, this huge original terrestrial carbon store would be about ten times the current estimated 38,000 Gt of DIC carbon in the oceans in 2020 report – https://essd.copernicus.org/articles/12/3269/2020/essd-12-3269-2020-f02-web.png.

The implied atmosphere<->surface ocean exchange of 80-90 Gt C/yr with net -2 Gt C/yr is often repeated, e.g. the latest ESSD (**2020**; fig. 2). Primary sources are such as Tran, Berry & Keeling (**1994**) that, however, merely speculates 85 Gt C/yr air<->sea exchange and net ranges of -0.2-2.4 and possibly a net outgassing of ±1.3 Gt C/yr to or from the surface ocean. None of these speculative or "hypothetical" data are supported by observations. The latter authors concluded "*The apparent disagreement between the surface disequilibrium method and the oceanic inventory method as well as substantial uncertainties in the application of both methods themselves preclude at this time any firm conclusions on bounds set by the oceanic isotope data on the ocean uptake of CO2*."

Regarding Ocean Contribution to NPP and Gas Exchange

Models of global carbon cycles are provided by NASA/NOAA (**2011**: **figure**) or IPCC (**2013**: fig. 6.1 or **2019**) from the Global Carbon Budget (2019, **2020**: fig. 2), etc. The latter reference cited "Ciais et al. (2013)" as its flux source that is also IPCC (**2013**: fig: 6.1) which itself quotes "(Sarmiento and Gruber, 2006)" as its source for ocean gas exchanges. Sarmiento & Gruber (**2006**: fig. 10.1.1) have outgassing of about 90 GtC/yr giving their source as "*Based on Sarmiento and Gruber [2002] and Sabine et al. [2004]*". Sarmiento & Gruber (**2002**: fig. 2) say their total numbers are from IPCC (**2001**: 197, fig. 3.1c) that from unspecified sources says: "*The annual two-way gross exchange of CO_2 between the atmosphere and surface ocean is about 90 PgC/yr, mediated by molecular diffusion across the air-sea interface. Net CO_2 transfer can occur whenever there is a partial pressure difference of CO_2 across this interface*." Thus it appears this citation cycle is self-referencing and circular without a factual basis check!

Another oftentimes quoted source of the 80-90 Gt C/yr gas exchange from air <–> ocean is given as "Siegenthaler U, Sarmiento JL (1993) Atmospheric carbon dioxide and the ocean. Nature 365(6442):119–125." The Nature article by Siegenthaler & Sarmiento (**1993**) is available online here: **https://www.gfdl.noaa.gov/bibliography/related_files/us9301.pdf**. Ironically, they give marine biological new production of just 10 Gt C/yr compared to terrestrial NPP of about 50 Gt C/yr, or five times as much on land! And data in their Figure 1 showing 90 GtC/yr passive ocean "gas transfer" (= outgassing) to air they say are "those given in the **1990 IPCC** assessment".

Looking through the IPCC (**1990**) assessment their source is Sundquist (**1985**: tab. 3) that has many references for air<-> sea gas exchange invasion/evasion fluxes estimates varying from 23-290 Gt C/yr! It appears that most subsequent quotes have used the values from Sundquist's table 3 of Bacastow & Keeling [1973] and Stuiver & Quay [**1981**] who use the Suess Effect to give a range of 82-91 Gt C/yr. Here my search ended as Bacastow & Keeling is a conference paper that is not readily accessible online, and Stuiver & Quay calculation is based on their data. Worryingly, Stuiver & Quay make no mention of "soil", "peat" nor "permafrost"...

Nevertheless, the original source for apparent ocean NPP values appears to be Revelle & Suess (**1957** - https://www.tandfonline.com/doi/pdf/10.3402/tellusa.v9i1.9075 : tab. 2 with data "*in part after HUTCHINSON (1954)*") giving photosynthesis on land as 73 **±** 18 Gt CO_2/yr and in ocean of 460 **±** 300 Gt CO_2/yr; converted from CO_2 to carbon by x 0.27 factor gives just 20 **±** 5 Gt C/yr on land and 124 **±** 81 Gt C/yr NPP in Ocean! The ocean range is thus 43-205 Gt C/yr! Yet they also admitted uncertainties of several orders of magnitude rendering this "***too uncertain to allow any definite conclusions***" but subsequently most often revised downwards. They also admit: "***Practically nothing is known about the C^{14} age of soils***". Significantly, 1957 is one year before empirical data collection started at Mauna Loa. It should be noted that Suess assumed from the outset (*a priori*) that ^{14}C depletion is due mainly "*from combustion of fossil fuels*" and ignored erosion of ancient soils.

Actually Hutchinson (1954: 380) gives NPP on land as just 20 **± 5**Gt C/yr but he admits there is, however little doubt that these figures are too low, as they fail to take into account tropical rainforests. For Oceans, (Hutchinson (1954: 380) has 126 **±** 82 Gt C/yr that may be "*an order of magnitude too high*". Indeed...

Yet, Hutchinson (1954) suggested the terrestrial biota appears to be a net source of CO_2 for the atmosphere, apparently he gained some ideas from V.I. Vernadsky's concepts of the biosphere. Woodwell et al. (1978) agreed saying: "*analysis shows through convergent lines of evidence that the biota is not a sink and may be a source of CO_2 as large as or larger than the fossil fuel source*." Woodwell et al. (1978: tab. 1) have NPP of marine 24.8 Gt C/yr and land 52.8 Gt C/yr.

These latter authors said: "*Because of the paucity and uncertainty of data the actual rate of release is elusive; under extreme conditions it could be as high as 18 x 10^{15} grams of carbon per year, or more than three times the annual release of carbon through combustion of fossil fuels, currently estimated as about 5 x 10^{15} g.* " This is similar to my current estimate!

Hutchinson, G. E. 1954. "The Biochemistry of the Terrestrial Atmosphere." In: G. P. Kuiper, ed., The Earth as a Planet, pp. 371–433. Chicago: University of Chicago Press. https://articles.adsabs.harvard.edu/full/1954eap..book..371H.

Woodwell GM, Whittaker RH, Reiners WA, Likens GE, Delwiche CC, Botkin DB. The biota and the world carbon budget. Science. 1978 Jan 13;199(4325):141-6. doi: 10.1126/science.199.4325.141. https://www.researchgate.net/profile/William-Reiners/publication/6027359_The_Biota_and_the_World_Carbon_Budget/links/5685ffc008ae051f9af1eef5/The-Biota-and-the-World-Carbon-Budget.pdf.

A final interlink of O_2 <-> CO_2 <-> H_2O, and to round off the false ocean claims, is the lack of any evidence for net nor seasonal flux uptake of carbon by oceans (**red**) in S polar regions. If anything, rather than absorb, more CO_2 is passively outgassed on average as oceans warm:-

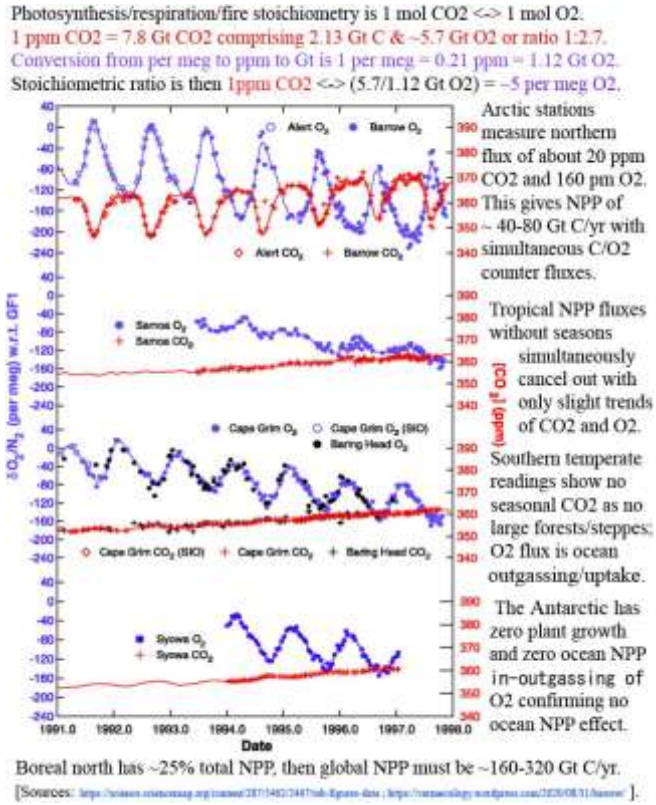


Figure source: https://vermecology.wordpress.com/2021/05/27/h2o/.

The final comment in the figure above alludes to the fact that data shows Boreal NPP of 60-80 Gt C/yr (median ~70 Gt C/yr) in its short but light summer period. For instance, Bartsev et al. (**2012**: fig. 3. – **https://core.ac.uk/download/pdf/82433487.pdf**) model a boreal flux of about ±62 Gt C/yr. Basile PhD thesis and her Basile et al. (2020 paper here:- https://bg.copernicus.org/articles/17/1293/2020/; their table 1 and figs. 3 & 4) support boreal observation of ±15.3 ppm flux with a CASA modelled NPP of ~40 ppm (= 85.2 Gt C that corresponds exactly to the observed Barrow flux of ±20 ppm and represents an NPP rate higher than the claims of global total of 50–60 Gt C NPP). Further, if Harverd et al. (**2020**) are correct that: "*land north of 35°N contributes less than 25% to global GPP)*", then total is (4 x 62 =) 248 Gt C/yr global NPP that exceeds my estimate of 218 Gt C/yr terrestrial NPP (Blakemore 2018) while yet allowing (248-218 =) 30 Gt C/yr ocean NPP.

Conversely, there is no supporting evidence for a substantial flux in oceanic NPP nor any pronounced effect on atmospheric gas exchange, e.g Piao et al. (2019: fig. 9 https://onlinelibrary.wiley.com/doi/full/10.1111/gcb.14884) show negligible, or negative, seasonal/annual contribution from the Ocean compared to Land to the total C flux.

It should also be emphasized that political hype for ocean acidification or plastic pollution are orders of magnitude greater in the soils that we depend upon for 99.7% of human food, etc. For example plastic pollution up to 23 (https://www.unep.org/news-and-stories/story/plastic-planet-how-tiny-plastic-particles-are-polluting-our-soil) or 32 (https://ieep.eu/news/isqaper-

exploring-plastic-pollution-in-soil) times greater in soils compared to the oceans, and ocean acidification of 30% in the last two centuries is trivial to soils acidification. On average this has decreased by 0.24-0.24 pH points globally (https://iopscience.iop.org/article/10.1088/1748-9326/10/2/024019/pdf ; https://iopscience.iop.org/article/10.1088/1748-9326/ab239c), but more so in intensive agricultural situations by up to two pH points or 10,000% in just decades (e.g. https://www.nature.com/articles/s41598-018-20873-5).

Furthermore, no marine fish nor coral has become extinct in the last 250 years whereas at least three earthworms have disappeared within this timescale (Blakemore 2022). Irreplaceable soil organisms are being lost at an unprecedented rate, once microbes are properly included the studies, estimated as up to 23 species lost per second (Tol...).

An outcome of context and triage is that claims oceans are important due to their provision of food is disproven as they only supply 0.3% of total human food, the ocean catch is at the highest on record and there are no reports of any fish or corals becoming extinct in the last 250 years. Neither do the oceans support biodiversity, biomass nor contribute to O_2 or CO_2. cycles. Conversely the soil is our major source for provisions and medicines. Topsoil is being destroyed and soil species lost at massive rates yet these issues are mostly ignored. A sea change in attitudes and priorities is overdue to consider the soil foundation and its fragility.

Regarding Research Funding Disparities

It is self evident that most research is on Oceans or Space (e.g. NOAA or NASA) obtaining most funding for research not only on carbon, but also on (astro)biota, and so on.

One example in Japan is JAMSTEC employing 1,000 staff with a 75 billion yen ($0.5 billion/yr) budget (www.jamstec.go.jp/e/about/suii/). With proper allocation of staff for context of soil:sea:sky there would be about 999 working on soil and just 1 person on all others. Yet the reverse seems true (although it is doubtful there is a single soil worker there). In the last 20 years JAMSTEC received about $10 billion support, this is enough to establish Soil Ecology Institutes throughout Japan, if not in all of Asia/Australasia, to have made some real progress.

NASA's annual budget is $14.86 billion/yr – or $42 million per day; NOAA only $7 billion/yr. The GBR in Australia often gets $ billions but seemingly not enough (https://theconversation.com/the-1-billion-great-barrier-reef-funding-is-nonsensical-australians-and-their-natural-wonder-deserve-so-much-better-175924). Yet not one Soil Ecology Institute exists, there is about zero funding for organic research and earthworms are lowest priority.

APPENDICKS D (rejection of original 30th November, 2022 manuscript MS land-2100500).

Timeline: Invited to submit abstract by Dr Diane Pearson on 29th August, 2022.
Submitted abstract 25th October, 2022.
Submitted MS (Land - 2100500) 30th November, 2022.
Editorial reviews went through two rounds with two referees and was accepted with:
"*Comments and Suggestions for Authors*
I think the paper had improved a lot. It can be accepted.
Submission Date
30 November 2022
Date of this review
27 Dec 2022 07:52:17."

Yet on 5th Jan., 2023 the editor, Dr Richard ("Dick") Asprinall, made a unilateral rejection for mostly invalid reasons. https://www.mdpi.com/journal/land/special_issues/LSF

Prof. Dr. Richard Aspinall **E-Mail** - rjasspinall10@gmail.com **Website**

SciProfiles *Guest Editor* Honorary Research Fellow, James Hutton Institute, Aberdeen AB15 8QH, UK **Interests:** land systems science; land use; GIS; sustainability; environmental change; landscape ecology.

"*Editor Decision*

Decision

Reject and encourage resubmission

Comments

I find that this manuscript is not acceptable for publication in LAND in its current form. I do note that the revised manuscript received one review (from Reviewer 2), and that this reviewer suggests that the manuscript can now be accepted for publication. However, this reviewer provided no written justification for this beyond ticking the default proforma questions. The other reviewer (Reviewer 1) declined the request to review the revised manuscript. Because of this I have examined hte manuscript closely, including against the comments and feedback provided by Reviewer 1. My decision on the revised manuscript is based on the extent to which these comments have been addressed, and on some aspects of he manuscript itself based on my reading of it. Key criticisms of the original manuscript from the two reviewers are: Reviewer 1. Although the manuscript brings some interesting numbers and estimates, the text is poorly written and not well organized. I recommend a strong revision throughout the entire manuscript. Reviewer 2. 8. Line 272-1125,the results and discussion should be reframed. I can not get the key point of the paper. There are too much results and discussion, but I can not find the relation with topic easily. The logical relations are confused. The author should summarize the results of other people and give the new point, which make the readers understand easily. Additionally, both Reviewers indicated that the Introduction is too long, and, indeed, that the paper is too long, and lacks clarity. 1. I note that the author submitted an abstract in October for pre-screening against the focus for the Special Issue "Landscapes and Sustainable Farming". This abstract (Heritage Data on Soil Organic Carbon and Biota Loss Points to Future Farm Restoration) was found potentially to fit within the rubric of the SI. Although the manuscript submitted has links to

the material described in the Abstract from October, the content appears to take a somewhat different focus and emphasis than the earlier abstract might have suggested. The manuscript is less explicitly focussed on the subjects within the SI on Landscapes and Sustainable Farming, despite its focus on the important issue of soil. My conclusion is that the manuscript as presented is not sufficiently within the scope of the SI, even though the original proposed abstract appeared to fall within the scope. 2. The feedback from both reviewers notes that the text is poorly written, not well organized, and confused. Reviewers recommended a strong revision and reframing. Unfortunately, I do not find the revised manuscript to have addressed this issue sufficiently. The paper, already noted as being too long and confused, has got longer after revision. This has not helped with the presentation of material, clarity for readers, or reporting of the work carried out. The manuscript still needs major revision. 3. Although the key questions are stated clearly in the Aims of the Current Study at the end of the Introduction, these now appear on page 10 of the revised manuscript. The Introduction ended with these aims on page 6 in the original manuscript (in both cases the LAND formatted pdf versions). The additional 4 pages of Introduction have not improved clarity. 4. It also appears that much of the material in the RESULTS & DISCUSSION (pages 11-39 in the pdf) is not results produced in the research, but reporting other sources. This also suggests that the manuscript still need major revision, with significant restructuring and greater clarity in focus. 5. The Material for the Supplementary Information (Appendices A-C, Strzeleki Excel file - see page 43 of the pdf) are still to be added. These need to be provided to be part of the Peer Review. As such, the submission is incomplete and cannot be fully reviewed in this state. 6. I am surprised that the manuscript and text makes no reference to: a. Jörn PW Scharlemann, Edmund VJ Tanner, Roland Hiederer & Valerie Kapos (2014) Global soil carbon: understanding and managing the largest terrestrial carbon pool, Carbon Management, 5:1, 81-91, DOI: 10.4155/cmt.13.77 b. The Global Soil Organic Carbon Map (FAO and ITPS. 2018. Global Soil Organic Carbon Map (GSOCmap) Technical Report. Rome. 162 pp) Link: https://www.fao.org/3/I8891EN/i8891en.pdf Apart from the apparent relevance to the subject of your paper, given the frequency with which you refer to there being no institution named "Soil Ecology Institute" (not only in this manuscript and your cover letters, but also in another paper of yours that I have read), the institutional make up of the partners is perhaps instructive. ***The report's contributors include 134 institutions from 83 countries. I would encourage you to note that many of them have programmes of soil ecology as an integral part of their wider programmatic focus***. [This is perhaps the most insulting and insidious comment trying to marginalize and diminish my work by saying all these other workers know better or do better – they don't as their report states "*Global Soil Organic Carbon Stock for topsoil (0 to 30 cm) is 680 Petagrams*" or just 680 Gt SOC compared to my true global estimate of 10,000–15,000 Gt SOC!! Which one of us is correct? Also, and this is key, nowhere in their 164 page report does it mention "Soil Ecology". Q.E.D.]. *To pick one example with which I am most familiar, the UK contributors include The James Hutton Institute. This was formed in 2011 from merger of the Macaulay Land Use Research Institute and the Scottish Crop Research Institute. MLURI was itself a 1987 merger of The Macaulay Institute for Soil Research (founded in 1930 and which included the Soil Survey for Scotand) and the Hill Farming Research Organisation. It is true that none of the component institutes had 'Soil Ecology' in its title, yet throughout the various institutional names and mergers, MISR, MLURI and JHI each has followed comprehensive programmes of research that include soil ecology along with the other themes they address. I suggest that having soil ecology as part of a wider programme is more constructive in addressing complex issues in a dynamic and interconnected world than having a pure and satisfyingly labelled, yet thematically-isolated, institute. 7. Some observations on content, framing, and use of evidence in the manuscript. I offer these as constructive feedback, and hope that they will be treated as such. 7.1 Many of the claims made are unsupported by data or citations. A trivial example, but one that is easy to find, is the claim made at the start of the manuscript (in the Abstract) that SOC is the basic metric of land. This is unsupported in the manuscript, although it is clearly the focus of the manuscript and of you as author. It is a statment that is also most certainly contested in the land science community - evidence for this being that it is seldom mentioned in papers on land science. The claim made is also different in tone from how it is framed in the October abstract where you claim that SOC is "A basic metric" (not "THE"). Clearly SOC is important - but not for the majority of land scientists who are the primary readership of papers published in LAND. 7.2*

Other claims are supported by long quotations. Reviewer 1 commented on this stating that the third paragraph was a citation of someone else's work. 7.3 The lesson of 7.1 and 7.2 is that improved use of citations as evidence to support your 'scene setting' in the Introduction will help to reduce the length of the Introduction and underpin the statements you make. 7.4 The manuscript repeatedly refers to your 2018b paper (Note too that this is cited as 2018 without the a or b suffix in Table 4), and many of the results are based on estimates linked to that work. Further, at the end of the manuscript you challenge others to "prove the Earth is mirror-flat or supply their own etimate of fine topography". More on ths below 7.5 Perhaps there are a couple of papers within the material in this manuscript? I suggest splitting the material as a mechanism to focus the content, improve and simplify communication, and help with publishing. Perhaps one focus could be a revised Carbon Budget - strip this out of the manuscript and write it up as a separate piece of work that you submit for peer review? A second paper could be more focussed on soil organic carbon in landscapes and at landscape scales (and, if for farming - with fit to the Special Issue) 7.6 Can you add error/sensitivity analysis to your Fermi method calculations? 7.7. Overall, to a large extent the manuscript comes across as rant about four things: One is a Soil Ecology Institute. You obviously feel strongly about the need for this, and I have mentioned an alternate view above. A second is the importance of SOC. Perhaps the suggestion of separate papers on a Global Carbon Budget and SOC in landscapes would address this? A third is the inadequacy of most of the major programmes focussed on environment, society and change (including the SDGs). Frankly, I would just drop this. A fourth is measurement of area on a topographically complex and multi-scale surface. Clearly you have the 2018b and 2020c papers and many values you have recalculated, possibly on the basis of doubling of area (it isn't always clear in the manuscript). Although I don't doubt your calculations, I might suggest that you could improve the nuance, meaning and explanation in your results both in relation to land use and other activities on the land, and in relation to soil characteristics, especially in relation to scale of both processes and measurement. The question of 'actual area' is reminiscent of the 1920s work by Lewis Fry Richardson on how long is the coastline of Britain? This was subsequently used by Mandelbrot in his work on fractals. Essentially, the length of the coastline depends on the scale at which it is measured. Measurement of the area of a topographically complex and multi-scale (fractal?) surface is similarly dependent on scale. Without claiming that the Earth is mirror flat at all spatial scales (clearly it isn't), I would contend that rugosity at scales of cm2 and mm2 (at which scales your papers claim area doubles and quadruples) are irrelevant for many applications and measurements in land use and land cover. Put simply using an example, one does not - and cannot - plough furrows and plant crops or trees at scales of cm or mm, so the multiplier on area represented at those scales is precision bias for land cover. The apparent greater precision is from measuring phenomena at a different scale, but not at a relevant scale for processes. On the other hand, these scales are relevant for understanding soil as a 3D material with many properties that operate on or at the (2D) surface of particles and soil structures. At present it is not possible to separate which of these apply to the many values you give in the paper. I suggest that there is a case for some properties on the land surface that the area mesured at fine scales of roughness does not increase their abundance, while for some other properties (of soils in particular) it is centrally important. You could be clearer about the limits and relevance of measurement of area at different scales in your manuscript. Relevant background information on scale and scaling might include Allen and Starr's book on Hierarchy Theory, and the use of Stommel diagrams (exemplified in Simon Levin's Macarthur Lecture published in 1992: The Problem of Pattern and Scale in Ecology: The Robert H. MacArthur Award Lecture, Ecology https://doi.org/10.2307/1941447. Both Hierarchy Theory and recognition of scaling are now centrally included within Landscape Ecology (which is the section of LAND to which you also submitted this manuscript).

Decision Date

4 January 2023"

My (RJB) response and resubmission 13th January, 2023:
"Reply to Editor's Comments on MS **LAND-2100500**

Thank you for your detailed review and useful suggestions to improve the MS. Please allow me to reply as succinctly as possible to each itemized point raised.

I quite agree the MS has grown considerably as it tries to covers much "ground" although it does comply with the October 2022 Abstract originally submitted. Moreover there are many new and important findings in this study, several quite surprising such as: Existing (flat) SOC values already much above those published; massive LUC losses of mainly vegetation and SOC losses to erosion (often these two published intermixed, hence much inherited confusion); our general ignorance of soil biota and alarming rates of extinction that are unreported. You may also note that, unlike many other publications on Problems, I also offer proven Solutions. The MS is now clearly modularized in sections that readers may select according to their interests, while also being coherent in its entirety.

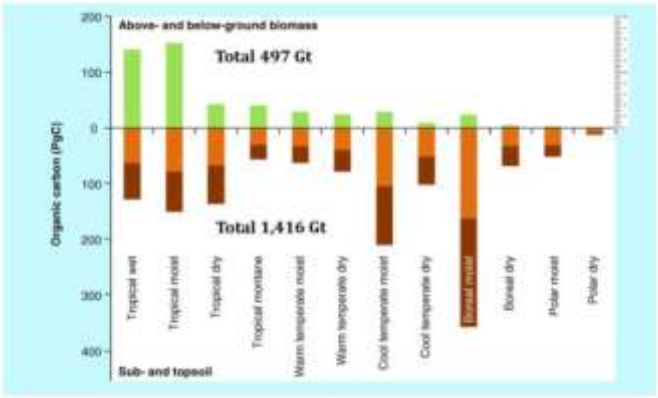
1 Heritage data aspect of the MS became less relevant with new LUC information, but the theme of "**Landscapes and Sustainable Farming**" are still the main focus when Landscape includes terrain and below-ground ecology, and Sustainable Farming includes organic with preservation of biodiversity.

2. Apologies that the text was poorly presented. I have now completely revised the format and flow.

3. The Introduction expanded when I realized that some of what I thought were new data were already published and available, thus I shifted this info from Results section to Introduction to help "Set the Scene" for what we already know and what we lack (or what we had and what we lost..).

4. Results & Discussion are extensive due to the amount of new information or existing information that is changed in subtle ways. Terrain doubles most land values, but not all (e.g., waterlogged peat).

5. Regarding no reference to Scharlemann et al. (2014) nor GSOCM map (2018). Sorry but these two sources exemplify the key issue: Scharlemann et al. https://www.tandfonline.com/doi/full/10.4155/cmt.13.77 have median global SOC sock of "*1437 Pg C*" and claim "*There is **consensus** that emissions from land use and land cover change (LULCC) are, after emissions from fossil fuel combustion, the second largest anthropogenic source of carbon into the atmosphere*". My study disproves this, but this was also already known from 1950s. Their figure 3 was already reviewed in my publication https://www.mdpi.com/2571-8789/2/4/64/htm fig. 11:



The problem is their 497 Gt C above-ground value is less than **Rodin et al. (1975) of 1,200 Gt C phytomass** - https://nap.nationalacademies.org/read/20114/chapter/3#23, also Crowther et al. 2019 of 595 Gt C; Wuepper et al. (2021) 601 Gt C while Blakemore (2018b) had 1,100 Gt C! Their below-ground SOC total of 1,416 Gt C is ridiculously low and incomplete. My total SOC is >10,000 Gt.

Ditto the FAO/ITS database https://www.fao.org/3/I8891EN/i8891en.pdf if you read it they only go 0-30 cm depth and for 1 km grids come to a "***Global Soil Organic Carbon Stock***"of **680** Petagrams. A truly pathetic grand total and begs the question: Why just 30 cm? Why not take 30 cm ocean C too?

For all other SOC estimations these also hover around 1,500 Gt C for 1 m. Interestingly, latest info from Wang et al. (2022: tab. 1) of flat ~1-km grid WISE vs. SoilGrids in top 2 m are 3,814.8 vs. 5,796.1 Gt SOC. Doubled for terrain, range is ~7,630–11,592.2 Gt SOC as new total values, as I indeed show.

But we do not need terrain – as I explain at the core of the study – with just a few examples: Jobbagy & Jackson (2000) reported 56% more SOC at 3 m compared to 1 m, ditto D'Elia et al. (2017) (i.e. 3,000 Gt C), and Harper & Tibbett (2013) had two to five times greater mineral SOC at depth (5 x 1,500 = 7,500 Gt C). Litter adds 300 Gt C, Roots another ~500 Gt C, Permafrost at least 3,000 and Peats doubles to >1,123 Gt C from Loisel et al. (2021), Nichols & Peteet (2019, 2021), not from me.

In answer to the simple question of total global SOC stock? Seemingly only my MS gives a true total.

Sorry I have to disagree with your statement that the "*contributors include 134 institutions from 83 countries. I would encourage you to note that many of them have programmes of soil ecology as an integral part of their wider programmatic focus*". Most are mere chemists. Many physicists or "soil engineers". Where they claim "ecology" it is mainly microbial and often only a minor part of their interests. Not one of them can give total topographical land surface area, total SOC, nor biodiversity.

Indeed I visited Macauley Institute when I worked >2 years on soils in Scotland (I also did field work at Pitlochry the only Taiga site in UK!) and there was no earthworm ecologist there then and there is not one there now to my ken. A few are scattered through a few universities in UK but there are none at any Museum. In BM is Emma Sherlock who may be keen but is not a PhD (I applied for that position decades ago but was rejected as being "overqualified" with PhD). She has a desk in the corner on one floor below Dr Alex Muir's Polychaete lab that takes up most of the floor above. I know as I visited there a few years ago. It seems no full-time earthworm workers remain elsewhere.

In contrast there are literally 100s of marine polychaete worm workers at most all Museums. Why?

Problem remains: not one SOIL ECOLOGY INSTITUTE. Nothing like NOAA, Scripps, Woods Hole, NIWA, etc.: https://en.wikipedia.org/wiki/List_of_oceanographic_institutions_and_programs.

We are literally so ignorant of our soil biota that I find new species every field trip. Just last year I published six new species from Japan/Okinawa, two from own my backyard! https://nh.kanagawa-museum.jp/www/contents/1646461957953/simple/B51_89-94_Blakemore_et_al_New_report_Japan.pdf

https://nh.kanagawa-museum.jp/www/contents/1646461957953/simple/B51_95-104_Blakemore_et_al_New_species.pdf

Neither of those papers was funded (actually I had to pay costs from my own savings). In fact, to be frank, I get zero funding for any of my work. This despite my free, online Series of Searchable Texts on Earthworm Biodiversity, Ecology and Systematics from Various Regions of the World – December, 2008 by Robert J. Blakemore: http://www.annelida.net/earthworm/ that includes a link to my COSMOPOLITAN EARTHWORMS book which is an ISO guide to all exotic earthworm in the World.

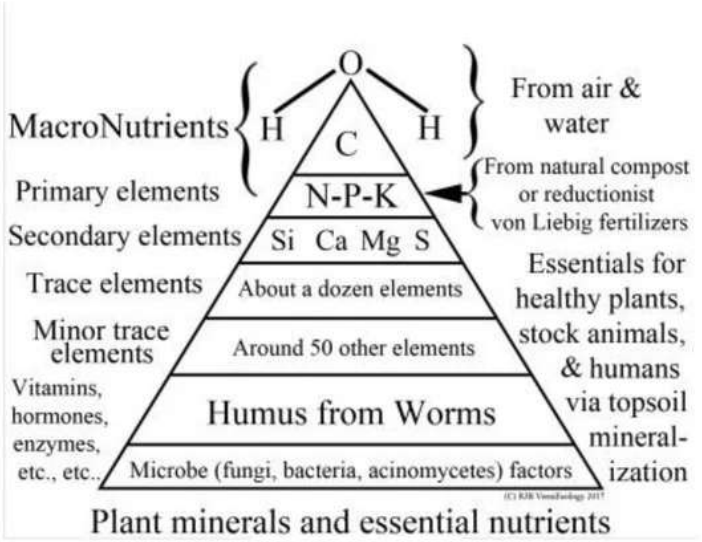
Probably because of such works, some people assume I am a mere earthworm taxonomist (well I have published about 500 new taxa or new records plus check-listed almost all known species and reviewed all 20 families with publication of three extinct earthworm species – one each from Tassie, NZ and Japan – with many more likely gone as per the IUCN Redlists I compiled for Japan and NZ).

But in reality I am an Agroecological Agronomist and Soil Ecologist. I actually grew up living and working on mixed dairy farms in Shropshire, roaming the same village where Darwin collected his samples when he was a kid. My grandfather was the Estate blacksmith and his family were farmers from the same area as Sir Albert Howard, another organic pioneer from Shropshire (and a couple of years ago in India I surveyed his Indore compost plots and Bhopal Soil Research Inst). My BSc Ecology hons project in 1981 was on Lady Eve Balfour's organic Haughley farm that I did as a tribute to Darwin's famous 1881 Earthworm book. My 1991 PhD was with CSIRO Tropical Crops & Pastures in Qld., and subsequently I studied Permaculture in

Tassie, researched at museums and taught as a uni professor in Asia for ~20 years. Not winding down, I still work today. But some help would be nice...

7. "*Some observations on content, framing, and use of evidence in the manuscript. I offer these as constructive feedback, and hope that they will be treated as such*." Yes, thank you. I was hoping for more of this from the referees. Especially any errors or omissions that I made in my findings.

7.1 SOC as this paper shows and as organic farmers know, ***is*** key to sustainability. And from my MS, interlinks to climate and global extinction rates. Land without SOC is called desert... Please see my fig. 19 Permaculture nutrient pyramid - https://www.mdpi.com/soilsystems/soilsystems-02-00064/article_deploy/html/images/soilsystems-02-00064-g019-550.jpg [image below]. So SOC ***does*** define soil. [



]

7.2 My "Other claims supported by long quotes" is because I agree with their statements and also this proves that it is not just my opinion. Many others have arrived at similar conclusions. However, they cannot have all facts because, as my paper shows, they do not have much basic soil information.

7.3 I have tried to reduce the Introduction by farming off to Appendices and summary tables/figs.

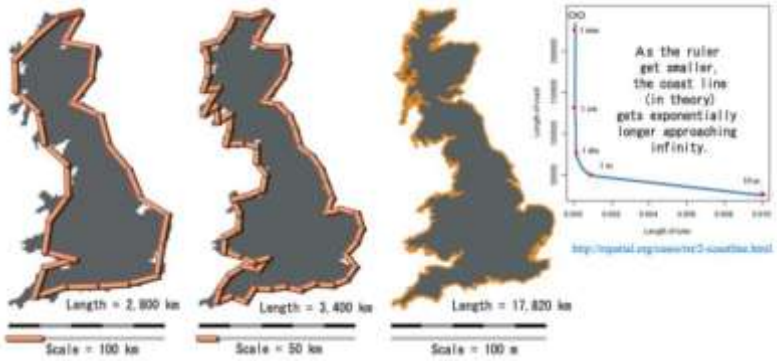
7.4 The manuscript repeatedly refers to my 2018b paper and I do challenge others to "prove the Earth is mirror-flat or supply their own estimate of fine topography". The reason for this is that if this information were available I would not have bothered to do all this work. There is no terrain for Earth. NASA will provide this for the Moon, Mars or Bathymetric Topography in exquisite m scale detail, but not for Earth. For ten years I tried to get an answer from them, or anyone. NASA told me they have the satellite data and offered to send it to me so that I can

compile it! Now if I contact them I get linked to the same person who gives me the same information of a flat earth surface area. You may care trying to get an answer..

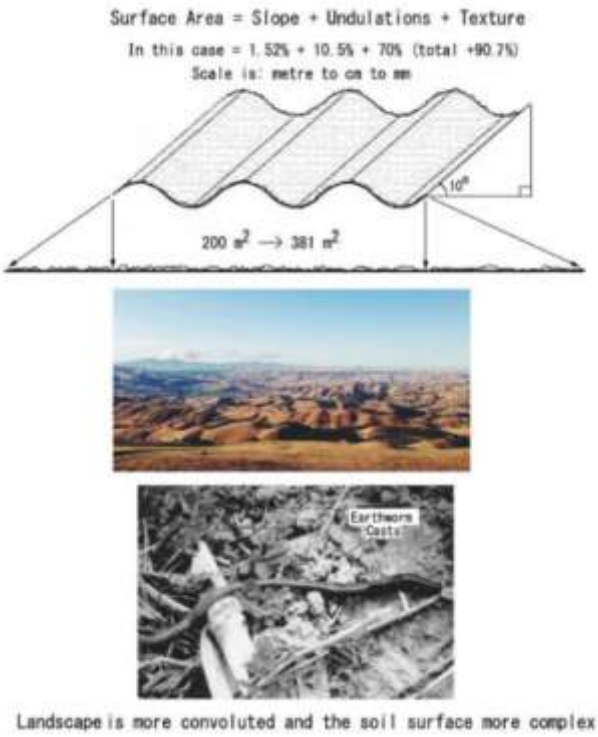
7.5 *Perhaps there are a couple of papers within the material in this manuscript I suggest splitting the material as a mechanism to focus the content, improve and simplify communication, and help with publishing*. Yes indeed, I thought this myself but here is the rub: I get no pay so cannot afford publication fees thus I cannot afford to submit to another journal. Fees are waivered on this SI and Land says there is no page limit. If my information is not published here if will likely not be published.

7.6 *Can you add error/sensitivity analysis to your Fermi method calculations*? Is this for the 2018 paper where I mention a Fermi estimate? Probably not as this is not my area of expertise. I am a Soil Ecologist and Earthworm eco-taxonomist not a mathematician nor geographer. To be frank I am not so interested in these arenas and have no skills. If someone else had done this work I would be happy to quote them. As it is, I can find no one else doing any such basic soil/terrain/eco research.

7.7. "*Overall, to a large extent the manuscript comes across as rant about four things"*: I would prefer to say it is a rational and well supported argument that: There is *no* SOIL ECOLOGY INSTITUTE. True! The SDGs as promoted as with most policies *do* mostly *ignore soil* (I would rather drop them too as I explain, but it is in the SI outline and FAO or IPCC etc. often refer to SDGs as justification). SOC *is* important yet ignored. Terrain *does* double land surface (unless you can disprove this). Yes, I know of Richardson & Mandelbrot as my paper shows https://www.mdpi.com/2571-8789/2/4/64/htm fig. A4:



Also, while the sea is "*level*", my fig. 3A shows how coarse terrain is **overlain** by finer scale rugosity:



Landscape is more convoluted and the soil surface more complex

You say: "*I would contend that rugosity at scales of cm2 and mm2 (at which scales your papers claim area doubles and quadruples) are irrelevant for many applications and measurements in land use and land cover. Put simply using an example, one does not - and cannot - plough furrows and plant crops or trees at scales of cm or mm, so the multiplier on area represented at those scales is precision bias for land cover.*" Sorry I have to disagree with you there as solar insolation measure is a Langley of one calorie cm^{-2}. The average seed or leaf area is mm^2-cm^2 as is the scale of an earthworm burrow or cast. Direct-drill or deep-ripping are organic techniques that use seed drills or tines of cm width. Anyway, such scale issues are already presented, justified and discussed in my 2018 terrain paper.

Again I am sorry; when talking of a flat-Earth you say "*clearly it isn't*" then you must agree that all oceanographers, land scientists and ecologists who say it is are not being factual? So, if the ocean is not 71%, just how much is it? 69%, 50% or, as I argue, just about 36% of Earth's total surface area?

About Hierarchy Theory or Strommel that referred, yet again, to oceans, I know nothing of unless it relates to Ecology and I note that scales start as sub-mm level, just like my Terrain and Biota... What I seek is true topographical soil surface area & total SOC, wondering why these aren't freely available?

I am happy to discuss these issues in more detail and to refine the MS further and would appreciate any support to achieve this. My best wish would be for the entire paper to be published and any objections or refinements to be made from that basis. Therefore I humbly resubmit a new draft MS.

As it is now, more than once I have given presentations saying earthworms are important and others have stood up to say sharks or deep sea organisms are important. Who are the audience to believe? [Who can possibly justify expensive deep sea or space search for life when soils are destroyed daily?].

I am also aware that my writing on SOC upsets the expert "soil scientists" who have overtly or covertly criticized my work without providing better data. E.g. 4p1000.org still promote 1,500 Gt SOC. Then there is the Soil Biodiversity Partnership that says soil supports about 25% of biodiversity. The Marine mob claims the Oceans support 80-99% of biodiversity. Whereas I show soil has >99.9%. Obviously something is wrong and I suggest this relates more to their funding rather than any facts. Such falsehoods will spread uncontested unless info, as in my MS, is publically aired for open debate.

Here is original October, 2022 Abstract that clearly following the latest MS theme and SI topic:

"Heritage Data on Soil Organic Carbon and Biota Loss Points to Future Farm Restoration
For "*Landscapes and Sustainable Farming*" in journal ***Land*** by Robert J. Blakemore [1,2]

Abstract
A basic metric is biotic soil organic carbon (SOC) yet global estimations range 1417–15,000 Gt C. Topographical terrain increases soil condition calculations and inventories on a non-flat Earth. Carbon contributions of soil biota are enumerated for earthworms, microbes, fungi, termites and ants. Loss of soil species and topsoil erosion are the most urgent, and the most ignored, of triaged concerns. Sustainable Development Goals (SDGs) have and will fail without a solid soil foundation. Historical sample records and compiled comparative data give present SOC conditions allowing an estimate contribution of topsoil loss to unrelenting increase in atmospheric CO_2. Although increased CO_2 has resulted in a global "greening" effect, it is argued this is outweighed by a more rapid erosion of topsoil and expansion of desertification due to excessive meat eating and unsustainable, soil extractive or toxic farm management. In particular excess synthetic Nitrogen acidifies topsoil and depletes soil carbon store dynamics. Soil restoration remedy is provided via composting, organic farming and Permaculture practice.

Thank you for your continued interest and support, for my work, for the soils and for the worms,

> *Rob Blakemore PhD Zama, Japan January, 2023 (New Year of the Rabbit, a soil megafauna)*"

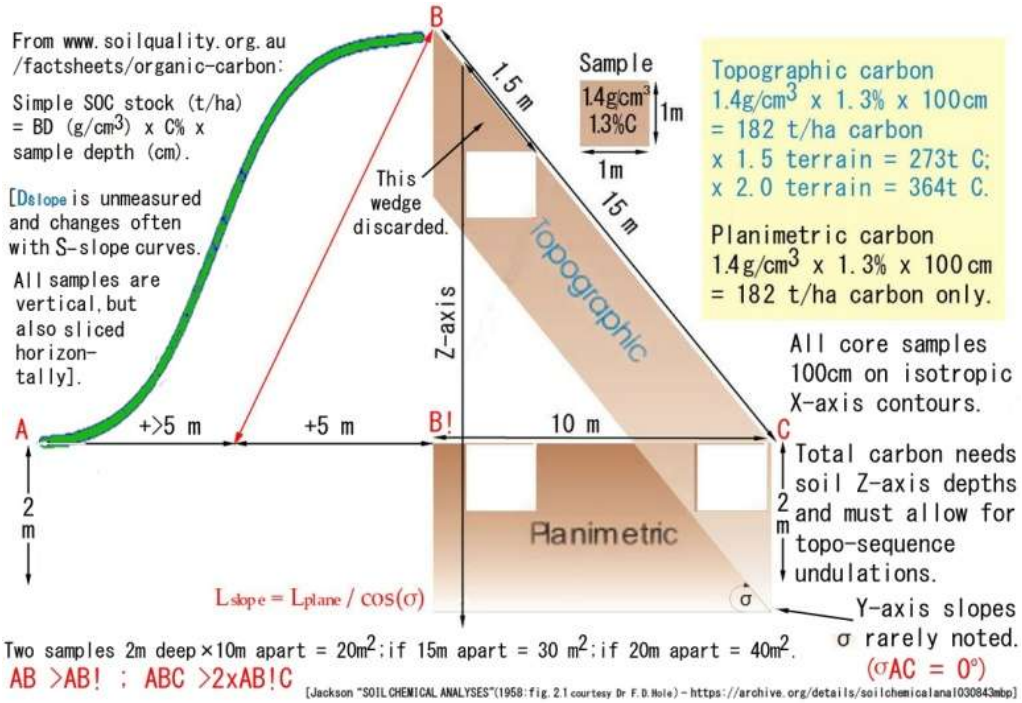
February, 2023 (six months after initial call for paper) decision still pending... So slow progress.

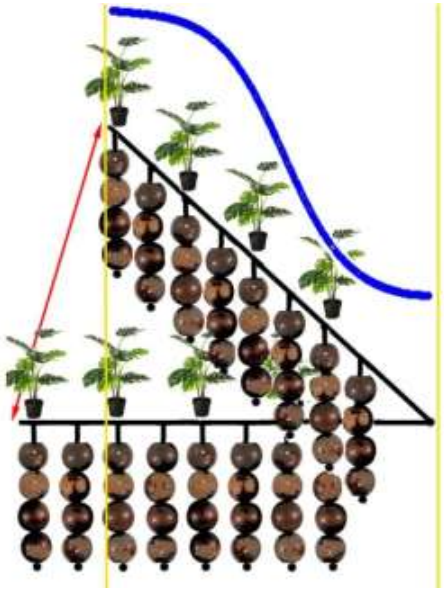
March, 2023 this Editor again rejected the paper despite another favourable Referee report.

My figures in reply to other comments that did not copy to journal's website are shown below:

"Comment following Blakemore (2018b) paper was uninvited (allowable) but misrepresented my work (not acceptable) as they claim samples were collected parallel to undulating soil

surfaces whereas it was actually stated more than once that they are taken "*perpendicular to the Centre of the Earth*" and soil samples are actually like the beads on a string-bead curtain being lifted or lowered:-





Please note too how plants, here symbolized by potted ferns, are closer together on slopes. This is what appears true to Nature with forests on slopes more dense than forests on plains.

The green/blue line is sigmoid as, in the real Geographical World, hills are curven not linear..."

8th March, 2023 the second referee accepted the second round or revisions and ticked all the positive boxes for accepting the publication. This is now two of four referees to have accepted the paper (one other referee did not complain to the revisions and one is yet pending..). The comments were: "*Comments and Suggestions for Authors*

The reorganization of the paper into 4 clear sections I found helpful and it made the paper more readable. I think this will be an important contribution, but it may also contribute to a healthy discussion as it goes against commonly held beliefs...and even policy issues being put forth."

The third referee (referee 2 of the second revision) comment on 19 Feb 2023 10:10:44 was: "*The topic of the paper is important and meets the scopes of LAND."*

08 Mar 2023. After two submissions of the MS and then six rounds of Reviewers' reports and after answering all Editors' comments a Reviewer again passed the paper.

"Manuscript ID land-2188984
Review Report Form

Quality of English Language

(x) English language and style are fine/minor spell check required

	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	(x)	( )	( )	( )
Are all the cited references relevant to the research?	(x)	( )	( )	( )
Is the research design appropriate?	(x)	( )	( )	( )
Are the methods adequately described?	(x)	( )	( )	( )
Are the results clearly presented?	(x)	( )	( )	( )
Are the conclusions supported by the results?	(x)	( )	( )	( )

Comments and Suggestions for Authors

The reorganization of the paper into 4 clear sections I found helpful and it made the paper more readable. I think this will be an important contribution, but it may also contribute to a healthy discussion as it goes against commonly held beliefs...and even policy issues being put forth.

The only thing that stood out to me as a statement that is not supportable is found in line 412-416 where the author states that organic food has more essential nutrients. He cites one study. This stands out because there is a body of literature that does not support this statement. Given the authors exhaustive evaluation of the literature throughout the paper, this statement seems odd...and as stated above, unsupportable.

Submission Date 13 January 2023 Date of this review 08 Mar 2023 13:00:12

Author's Reply March 2023:

Thank you for helpful comments earlier and as above. The whole MS is now checked for minor spelling errors and any other omissions. Regarding the mineral contents of organic foods – my apologies as I included a link to the wrong paper/authors. Rather than "Mayer, A.B., Trenchard, L., Rayns, F. (2022)" I mistakenly had "Mayer M., Krause H.M., Fliessbach A., Mäder P., Stefens M. (2022)". Now corrected, plus I added supporting info from (Thomas 2007: tab. 9) based on Review of the 6th Edition of McCance & Widdowson; and I also discuss Fan et al. (2008) study at Rothamsted.

Kind regards,

Rob Blakemore"

Hopefully it can now be accepted by the editor too...

Seems not... Jennifer Xu jennifer.xu@mdpi.com emailed me on 15th March, 2023 saying: "*to avoid any potential conflict of interest, they felt it is inappropriate for them to be involved in the editorial process, and they requested that the Editor-in-Chief team be invited to continue working on this manuscript from now on*". So, as I suspected, there is an editor conflict of interest/intellect. April, 2023 still waiting... No news... This really is shameful and slack.

Yeah-no, one month later got this curt and ridiculous rejection notice (my response in red)**.**

Academic Editor Notes (12th April 2023)

Dear Dr Blakemore,

First of all apologies for the comparably long time it took to come to a final decision for your submitted manuscript. The reason was that we have been receiving partly contradicting and hard-to-interpret reviewer assessments and related author reaction.

Therefore we now invited one of our editorial board member with specific soil science background to give us feedback about the quality of the manuscript. After a careful check, including also the prior reviewers' comments, we came to the conclusion to reject your submission in its current form.

The main reasons will be provided in the following, but please consider also the other reviewers' comments.

- The manuscript paper is too long (66 pages) (Land journal states there is no page limit and, as it is electronic publication, the length of the MS seems unimportant; also if the paper were to be divided into two separate articles surely the length total would be longer not shorter?) and would strongly need refocus. It was already thoroughly revised, clearly divided into four PARTS with CONCLUSIONS.

1. On the title:

Biotic SOC Stock: What We Had & What We Lost

- The term "biotic" seems unnecessary as organic is stated in SOC (Surely a minor point, but this is to emphasize that soil **is** a living biotic entity rather than just a chemical condition as for example claimed by Elhacham et al. (2020) who state: *"soil carbon is not living biomass".*
- Title should be: "Soil organic carbon stocks; what we…

2. On the abstract

There are many different ways of writing an abstract and an Introduction. This depends on the

academic subject involved, the journal itself and the specific topic of the article. It is important for the purpose of the research that authors can identify the patterns used in abstracts of comparable articles published in the same area, and for journals that authors might write for.

Abstract
A. Topic sentence (s) on the subject (its importance) and research question(s): what is(are) the research gaps in this field of research? "*Land's basic metric is soil organic carbon (SOC) yet global estimates range 1,417–15,000 Gt C*." That is they are out by an order of magnitude (a factor of x 10)
B. Objectives of the study "*Re-evaluation of topographical terrain on a non-flat Earth*..."
C. Materials and methods used in the study "*...increases most soil dynamic inventories. Carbon credits of our neglected and disappearing SOC stocks are enumerated*"
D. Main results (with quantitative information, tests of significance) Total stock ranges **are** provided.
E. Conclusions: how these results respond to the objectives; general implications of the research The CONCLUSIONS section has two tables with Prior and Post data, with a summary of the implications.

Please focus the different parts of the abstract on what we had & what we lost. Section 1.8 of the Intro clearly states study aims to re-evaluate SOC Stocks, SOC losses, and suggest Restoration options.

3. Content of the manuscript and length (as noted, there is no Land page limit also the topic covered are immense and refer to IPCC and ESSD report that themselves are up to 4,000 pages long!).
The paper should be concise and comprehensive, we recommend to shorten it to 10-15 pages maximum. (This is difficult as it requires omitting key supporting information or whole sections, this would exclude topics that may be of interest to different researchers also any objections are covered).
First of all, I would suggest to present data that are only supportive of the title. The introduction should introduce the definition of SOC stocks: what is taken into account. Please note that when stating SOC stocks, we do not expect to see data on fluxes between the different compartments but data on SOC stocks per see, i.e. on the different pools (that need to be defined) at maximum, not on above ground biomass, …, soil erosion. (I constantly refer to "context". Indeed the definitions of SOC stocks differ in different accounts, e.g. to only 0-30 cm depth or excluding roots (living or dead?) or totally ignoring peatlands or Permafrosts (!). My MS aims to be comprehensive with context of uniquely comparing organic carbon stocks in other biotic Realms for complete thoroughness. No one else seems to provide these data and it is concerning that such information is scattered widely and is inaccessible.

If soil erosion data are to be included (which I think constitutes a very original aspect of this paper), it requires another title." & What We Lost?" Please note that studies based on modelling or "guesses" are different than studies based on field data (see the 1.6 GTC displaced from its original place by Muller-Nedebock et al 2015; 80% or so being redeposited nearby). These data should appear in the discussion. These authors' quote: "*the total amount of SOC displaced annually by sheet erosion from its source would be 1.32 ± 0.20 Gt C*" which is less than the 4–6 Gt C/yr with 70% redistributed according to Lal (2006: fig. 3.2) as I show in my Figure 2. Please note too that those authors' also claim only 1,500 Gt SOC total and that GPP is just 120 Gt C/yr (i.e. on an unrealistic flat-Earth basis!).

If soil biota constitutes a pool, then the data presented should only be on soil biota, not above ground biota. Sorry, but Ecology interlinks and the below-ground is proportionate to the above-ground not least in the relevance and annual litter deposition that adds to SOC dynamics annually.

Then to me the only data to be presented here should be the evolution of: (1) soil organic matter stocks; (2) below ground biomass; (3) soil biota biomass I did all of that, uniquely with much

new and important information, plus I provide context with all aquatic and atmospheric biota too. This information alone merits publication of my data as it is not to be found in any other compilation.

The Introdction will then aim at pointing to discrepancies between the studies estimating the different pools and the paper should aim at depicting between what was quite speculative (modelling?) and based on field data. Yes indeed. The Introduction has the information know up to my review. And I am also a field ecologist. I go to the field and take samples, literally me and my students have taken 1,000s so I do know a bit about what I talk of.

The authors could then discuss the underlying reasons, but please note that authors such as Popleau et al (2016); Kirkby et al. (2013), Chaplot and Smith (2023) demonstrate that lack of key nutrients in soils as compared to nutrient exports by grains and other agricultural products is the main cause of SOC depletion by agriculture. Sorry this is simply not supported by the all the data I present in PART 3 on "3.13.2. Long-Term Experimental (LTE) Fertilizer Results for SOC, Yield, Biodiversity, Etc.". Please also note that other studies have shown an increase of SOC stocks following mineral fertilization and that there seems not to exist an upper limit for SOC stocks (see Rambaut et al 2022).
Sorry again, if you actually read my paper you will see that I covered this already (which is why the paper is rather long, to avoid such obvious objections: "***An assertion often made is that synthetic N fertilization maintains or increases SOC by enhancing production of crop residues, yet the opposite effect was realized in early days (e.g., Howard 1945, Balfour 1943). This is consistent with evidence that adding N enhances microbial decomposition of plant residues (e.g., Mulvaney et al. 2009) hence actually reducing SOC. Depletion of soil carbon exacerbated by adding synthetic Nitrogen deliberately or vicariously via global rainfall pollution. A meta-analysis on 257 studies on effect of soil N addition on soil had substantial stimulation of soil respiration (16%) and an enhancement in soil C mineralization (6%) within agricultural ecosystems (Lu et al. 2011); however, variable results were claimed from pot and field trials in Canada (Gagnon et al. 2016). Such information may be verified with reference to LTE results***."
4. Some other detailed comments on the style and way to present data and arguments. We can understand the author's passion for the subject but he should stick to facts. Sorry again but this is quite insulting. I have researched soils since 1980 (BSc Ecology) up through PhD in Tropical Agroecology and up to the current time. Thus I am a dedicated scientist "passionate" only about the truth and facts not at all interested in politics or profits. Please tell me if any of my statements are unsupported by published facts. If I am mistaken I will happily correct as I am quite disinterested in the outcomes. Nevertheless, soil erosion losses are much higher than pre-
467 various estimates, and a reasonable terrain-based dry rate is then in the order of 150–400
468 (median 275) Gt/yr.
- To be supported as all data and statements These data, as are fully justified in the paragraph, are from Lal (2006: tab. 3.2) of 200 Gt/yr and FAO (2015: 1-1) up to 200 Gt/yr, so may be reasonable.
Despite this, Lal (2021) noted: "The GCB [ESSD's Global Carbon Budget] 479 is strongly affected by accelerated erosion... However, the C emitted by erosion is not accounted for 480 in the GCB."
- Should start paper with such kind of statement in the Introduction if paper were on SOC erosion
This statement leads on directly to the next section: "*3.10. Reconciling Land Use Change (LUC) and SOC Loss with FF Emission Since Industrial Age*" that specifically discusses the ESSD (2022).
They 487 fail to say by whom and, in their list of 100 or so authors, only one seems affiliated with 488 any kind of "Soil" research. - Unnecessary type of comment
Sorry, but you seem to refer to a much earlier version of the MS which makes me suspect that you not seen the current version - https://susy.mdpi.com/user/manuscripts/displayFile/9deab95f96a838a282aa4bdd7af00a47 . **THIS IS A CONCERN** as it suggests you are reviewing a much earlier version that was corrected and fully revised after the last round of Reviews and justifies an Appeal for a proper

review. [Please also note that I sent a further revised copy to editor Jennifer Xu earlier this week with changes following last round from 8th March, 2023 that it was not possible to reply to until actions today (12th April, 2023)].
Figure 5. Oganic carbon cycle on land IPCC (2001: fig. 3–1 WGI-TAR1) ??
- We know from the 90s that 50 to 90 % SOC is formed due to root activity, providing enough nutrients are available Sorry, most studies are of "litter fall" and, according to Mathews (1997) belowground litter is "*rarely included"* and she further noted "*including standing and fallen dead wood may increase estimates of the fine litter pool by ~40%*" as I cover in "3.2.3.2. Litter, Logs and Roots in Above- and Below-ground "*Detritus*""..
Figure 7. Vegetation, soil carbon and soil microbes (adapted from Crowther et al. 2019: fig. 2). The 722 authors are explicit: "aboveground plant biomass (green) and soil carbon stocks (brown)".
- This is a beautiful pic
- We would love to see o it the curves for soil erosion (if the paper was on soil erosion) OK but you do realize that my comments and correction of all the values on that figure include a dotted line that I added to indicate not only what SOC Stocks may have been depleted, but what may be Restored...

Thanks for your understanding and best regards,
Sorry again but I am not particularly accepting these comments as the original paper was submitted in November 2022 in response to a request from Land editors for a Special Edition (with fee waivers). and went through one rejection by a single editor already (for reasons that mainly did not relate to the paper but referred to my 2018 MDPI publications) and that it and the current version have now been though six rounds of favourable anonymous Reviewer reports with all objections answered. All this now does is block this study (with about four years of my own research and reference checking) from being presented for open and public reception. Why? It makes little difference to the Land journal. It makes a difference to progress of Science which is pointless unless information is openly available...

Sincerely,
R. J. Blakemore PhD 12th April, 2023

Benjamin Burkhard
(Section Editor-in-Chief of the Section "Landscape Ecology" in Land)"

Thus, since August 2022 when I was invited to submit a fee-waivered paper that it took me several months (after several years of work) to do was rejected. The final notice showed publication fees of CHF (Swiss Franks) 2,450, which for two papers would be CHF 4,900!

There is a scam in business called "Bait & Switch". Seems reminiscent...

Anyways, this shows how corrupt, compromised & deprived of integrity Science now is. The bad guys win. Also Geography (just like Domestic "Science" or Sports "Science") is not real Science. More like a Socio-political exercise with no facts, just personal feelings and alternate truths. Welcome to the post-fact and post-Truth world. We are all left alone and adrift.

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https://www.tcm.com/video/281111/gone-with-the-wind-1939-movie-clip-frankly-my-dear

"*Do you mean to tell me, Katie Scarlett O'Hara, that Tara, that land, doesn't mean anything to you? Why, land is the only thing in the world worth workin' for, worth fightin' for, worth dyin' for, because it's the only thing that lasts*."

The End.